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Interactive comment on "Climate change and ocean acidification impacts on lower trophic levels and the export of organic carbon to the deep ocean" *by* A. Yool et al.

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First of all, we would like to thank our referees for their detailed and helpful comments. We are particularly grateful for those suggestions that had led to the production of new material for the manuscript (text, figures and tables), and feel that these have improved its utility. We have tried to address all of the points raised, and are happy to provide further clarification as needed.

For reference, the supplementary zipfile contains the revised text (as LaTeX), one replacement figure (for Figure 1), plus several new figures (as EPS) that are described in our response below.

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Note that, in the following text, referee comments are *italicised* while our responses appear in normal font.

Referee 1

Appendix A.1 that is supposed to describe the model's state variables is missing.

Appendix A1 is present, but is actually a table and should have been referred to as such. This has been amended.

On pages 3489 and 3490 the "d" in the derivative is missing in the denominator.

This is an error, and has been amended.

Also, why do some equations use the "partial"l symbol and others use the "d"?

In the case of MEDUSA-2's main state variables, the equations shown are partial differential equations that consider only the derivative of these variables in time, and ignore those in space. In the case of MEDUSA-2's benthic reservoirs, the equations shown are ordinary differential equations since there are no spatial connections between adjacent reservoirs. Each reservoir at a given spatial point is effectively isolated from all other reservoirs. As such, denoting these equations as ordinary differential equations is correct.

Finally, none of the equations are correct without adding to the time tendency terms the effect of the tracer-flux convergence due to advection and diffusion.

As is conventional, the partial differential equations reproduced relate solely to biogeochemical operations in time. However, to avoid confusion, a line has been added to draw the reader's attention to this.

1) Given the importance of changes in stratification to the results presented in the paper it would useful for reader to be able to see in section 3.1 plots that compare some measure of the model's pycnocline, nutricline and mixed layer depths to their counterparts for the real ocean.

A new figure showing WOA-derived and NEMO mixed layer depth, pycnocline depth and nutricline depth has been prepared and added to the manuscript. A paragraph describing this intercomparison has been added to the text.

“SST is only one aspect of the physical ocean that influences ocean biology. Of greater importance is the vertical structure of the ocean since this governs the availability of light and nutrients for phytoplankton. Figure X illustrates this by comparing annual average fields of observational and NEMO mixed layer depth, pycnocline depth and nutricline depth. Observational fields are derived here from the WOA while, similarly to Figure V, model fields are averaged for the period 2000-2009. Mixed layer depth is calculated here using a temperature criterion of 0.5C (Monterey Levitus, 1997), and NEMO’s global mean of 133m is close to that calculated from the WOA, 127m. Nonetheless, there are a number of differences, with NEMO typically overestimating the size of areas of deep mixing at high latitudes, as well as areas of shallow mixed layer depth at tropical latitudes. Pycnocline depth is calculated using the method of Gnanadesikan et al. (2002), and geographical agreement between NEMO and that estimated from the WOA is generally better than that of MLD. At the global scale, average pycnocline depth is 570m in NEMO and 584m in the WOA. Finally, while NEMO’s nutricline shows broad agreement with the WOA in terms of spatial patterns, there are significant differences, most notably within the subtropical gyres, where NEMO’s nutricline can be several hundred metres shallower than that estimated from the WOA. However, estimating nutricline depth from the WOA is complicated by its relatively low vertical resolution, and by limited data availability in some regions (note the blotches caused by interpolation in the Southern Ocean). NEMO’s global average nutricline is 172m compared to 174m estimated from the WOA.”

Additionally, a section of text and a further plot has been added to the results section to expand on how these properties change across the 21st century.

“Figure X further illustrates the changes in MLD, together with those in pycnocline and nutricline depths. At global scale, MLD decreases from a 1990s average of 135m to a

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2090s average of 113m, and while there is considerable scatter in local changes, it is clear that there is a systematic and significant shallowing of MLD in those regions which previously had deep mixed layers. Pycnocline depths also decrease from a 1990s average of 574m to a 2090s average of 520m. Figure X shows that while changes are not as large as for MLD, there is a tendency for regions with deeper pycnoclines (> 500m) to experience the largest absolute shallowing. Changes in nutricline depth are different again, though the 1990s average of 174m also declines, to 165m, by the 2090s. However, as Figure X shows, shallow nutriclines (< 200m) tend to deepen from the 1990s to the 2090s, while deep nutriclines (> 300m) instead tend to shoal, though there is a degree of scatter about this general pattern."

2) In section 3.2 is it possible to diagnose the relative importance of the different effects that contribute to the expansion of suboxic waters. In particular how much of the reduction is due to the reduced O2 solubility due to warmer waters? How much is due to the reduced ventilation as a result of increased stratification? How much is due to the shallower remineralization in response to the decrease in ballasting? To what extent does the reduction in productivity compensate for the other effects?

Unfortunately, it would require considerable effort – both further simulation and extensive analysis of oxygen budgets – to track back the cause(s) of the decline of oxygen. However, we have added a section of text to qualitatively expand on the cause of decline:

"Since the simulation forecasts lower export production into the future, subsurface oxygen demand by remineralisation is also reduced, which should instead favour higher concentrations. However, the decline in dissolved oxygen is driven in part by stronger stratification and weaker mixing that act to decrease ventilation of the ocean interior, and in part by warmer SSTs that decrease the saturation concentrations of oxygen in newly formed water masses."

3) For some of the figures, the differences between 1990s and 2090s are subtle and *it would be nice if the paper showed the percent difference using a blue-wight-red colormap.*

In drafting our manuscript, we originally explored the use of delta plots. However, we found that it was difficult to maintain a consistent format between plotted fields because of variability in whether relative or absolute difference was best. As such, we adopted the format currently in use.

page 3468 line 21: delete "change"

Amended.

line 24: delete "are"

Amended.

page 3470 lines 5-6 "with partic- ular declines in the equatorial Pacific and, particularly, the North Atlantic" could be rewriten more clearly as "particularly in the equatorial Pacific and North Atlantic."

In order that the extreme change in the North Atlantic remains clear, this has been amended to:

“DIN concentrations are uniformly decreased, particularly in the equatorial Pacific and, especially, the North Atlantic.”

line 10 change "region" to "regional"

Amended.

line 26 change "Switching from nutrients," to "In contrast to nutrient concentrations,"

Amended.

page 3471 line 26-27 "This is most marked in the Atlantic (−27.5 %) and the Arctic (−31.7 %) oceans." It is not clear what the numbers in parenthesis mean.

Yes, this is rather opaque. It has been amended to:

“In terms of where productivity lies vertically, the lower panels of Fig. 12 show that, almost universally, less production occurs within the mixed layer by the 2090s. This is most marked in the Atlantic (-27.5%) and the Arctic (-31.7%) oceans.”

page 3472 lines 8-14 what is the cause of the shallower remineralization?

This rather unclear sentence has been amended:

“This is indicative of shallower remineralisation, driven in part by warmer ocean temperatures and faster recycling, and in part by changes to detrital ballasting (see below).”

page 3474 line 7: add the word "In" at the beginning of the sentence.

Amended.

line 17: "...change in Table 2 that of CaCO3 production" should be "change

in Table 2 being that of CaCO3 production", i.e. add the word "being"

Amended.

page 3477 line 2: change "unfavorable" to "declinging", the meaning of unfavorable is ambiguous

Amended.

line 11: It is not clear what "(68 % to 64 %)" is refereing to, presumably it is refering to the productuion above the mixed layer but this isn’t clear.

Yes, this is also rather opaque. Its reference to mixed layer / below mixed layer production has been reversed, and the text has been amended to:

“... and with a smaller fraction of production occurring within the mixed layer in response to increasing ocean stratification (68% -> 64%).”

line 15 and 16 "In parallel, the volume of suboxic ocean waters increased by 13 %."

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It would be nice to see how the model's volume of the suboxic waters compares to observations.

We have calculated the corresponding numbers from the WOA and have amended the text to include them. Because oxygen is referred to earlier in the text, this amendment is made in the Results section.

"In passing, note that MEDUSA-2 overestimates the present-day (2000-2009) volume of suboxic water relative to the WOA (30 million km3 vs. 13 million km3)."

Referee 2

It is also necessary that the model results obtained are critically evaluated with respect to how the inherent assumptions and parameterizations in the model affect the outcome of the climate change model experiments. I feel that in this latter respect the authors could have done a bit more. The authors freely admit that their predictions of the decrease of calcification and of the changes in deep export critically depend on the chosen parameterization of the pH- dependent CaCO3 production and on the assumption of a strong regulatory role of biominerals for the fate of organic carbon (p. 3479 and 3480). The role of the first parameterization is explored by a control experiment without pH-induced changes in CaCO3-production.

The influence of the second parameterization is not really explored. I guess it would be overly difficult to design another control experiment to isolate the effect of this parameterization without changing the present-day model results, but the authors could have provided a bit more information, e.g. on

1. how much does the remineralization rate or length scale change through the reduced CaCO3 content and how does that compare with present-day variability in the length scale (or Martin-curve k)?

The referee raises an excellent question. As, for instance, Buesseler et al. (2007) or Henson et al. (2012) show, estimated remineralisation length scales can be extremely variable. To resolve this, we have estimated b values from our combined slow- and fast-sinking detrital fluxes at 100m and 1000m for both the 1990s and 2090s, and can compare the change between these two time periods with the interannual variability and spatial variability within the two decadal periods. A new figure has been added, together with this text in the Discussion.

“By way of representing how these changes relate to natural, or background, variability in export production, Fig. X illustrates spatial variability and how this changes between the 1990s and 2090s. Here, export is characterised by calculating Martin et al. (1987)’s critical b parameter, which effectively sets the exponential decay of sinking organic carbon and the vertical pattern of remineralisation. While originally assigned a constant value, 0.858, from a relatively limited dataset, subsequent work has found b to be significantly geographically variable - for example, Buessler et al. (2007) report values of 1.33 and 0.51 for neighbouring North Pacific sites, while the global synthesis of Henson et al. (2012) reports a broad geographical range from 0.24 to 1.18. Based on the change in total organic carbon fluxes between 100 m and 1000 m, and weighting spatially by flux magnitude, Medusa-2.0 has a average b of 1.342 in the 1990s, and 1.524 in the 2090s. To illustrate variability, Fig. Xa shows the spread of simulated b values for both the 1990s and 2090s, while Fig. Xb shows the corresponding distribution of change in b across the 21st century. In Medusa-2.0, b has a peaked, non-unimodal distribution, that broadly persists but shifts upwards between the 1990s and 2090s. This is congruent with results above that the sinking flux of organic material is attenuated more rapidly with depth into the future. The mode change in b across the 21st century is 0.125, though it ranges widely, and some regions experience a decrease in b - i.e. a larger proportion, but not necessarily a larger absolute amount, of sinking material reaches deeper into the ocean interior. As already noted above, this change is strongly dependent on the use of ballast-based export remineralisation in Medusa-2.0."

2. is the reduction in CaCO3 production the dominating effect for the reduction in export, or is there also a contribution from changing water-column dissolution in the upper ocean?

Yes, the OA-driven declining rain ratio of fast-sinking detritus is the dominant source of the decrease in the supply of organic carbon to the deep ocean. In shallow waters, where both slow- and fast-sinking detritus reach the seafloor, accelerated remineralisation of the former caused by elevated ocean temperatures will also decrease the supply of organic carbon to the seafloor. A new section of text has been added to reflect this.

"However, these factors - changes in Corg, opal, CaCO3 and the CCD - do not act equally, and Fig. X shows an estimate of their separate impacts. Global average Corg flux down the water column is calculated by applying Medusa-2.0's ballast submodel to 100m fluxes of Corg, opal and CaCO3 in fast-sinking detritus (slow- sinking detritus is ignored here because it plays a limited role away from the surface ocean). Fig. Xa first shows the change in the estimated normalised (to 100m) flux of Corg from the 1980s through to 2100. As described previously, the attenuation of this flux changes significantly across the 21st century. Fig. Xb then shows the profile of normalised flux for the 1990s, the 2090s and for four variants of the 2090s in which 1990s Corg, opal, CaCO3 and the CCD are substituted for their 2090s values. The intention being to determine which of these is most responsible for the decline seen in Fig. Xa. To supplement this, Table Y reports the estimated values of fast-sinking detritus at horizons down the water column for the same calculations, with the final row reporting the flux of organic carbon at 5000m normalised to that at 100m. This analysis finds that the dominant factor in the change in the deep flux of organic carbon over the 21st century is the loss of ballasting CaCO3. Opal and the CCD play very minor roles and the "starting" flux of organic carbon plays practically no role, since the ballast submodel quickly attenuates the excess 1990s Corg (to the point that the normalised flux at 5000m is lower than the case using 2090s Corg). As such, this further emphasises the importance of understanding both the veracity of export submodels based around ballasting and OA impacts on a key driver of these, calcification.

Note that the situation is slightly more complicated in shallow regions of the ocean, where slow-sinking detritus can persist long enough to reach the seafloor. In these regions, elevated ocean temperatures act to increase the rate of remineralisation to provide a separate mechanism by which the food supply to benthic communities is decreased."

3. the authors mention changes in the opal production also: How much does that contribute to the changes in export production?

As the text added in response to the above question points out, the impact of opal production decline (which is proportionally smaller than the decline of CaCO3 production) is minor relative to that of CaCO3.

The authors also mention at least two other effects that contribute to the feedbacks, but without further discussion, namely that in their scenario runs the Si:N ratio of diatoms changes, and that the increasing temperature increases metabolic rates. In both cases, not even the way that these effects are parameterized is presented in the paper and the reader is referred to a companion discussion paper. Given that the paper is on feedback effects, it would have been nice to discuss the parameterizations and to show briefly whther they are not that important for the effects discussed here.

Text has been added to the description section to note the model's use of the Eppley (1972) temperature response (see later), and a variable Si:N stoichiometry for diatoms (see later also):

"Though model diatoms have a fixed C:N ratio, Medusa-2.0 includes a separate state variable for diatom opal, allowing a dynamic Si:N ratio. This allows Medusa-2.0 to reflect known diatom responses to the availability of silicic acid and other nutrients (Mongin et al., 2006)."

Regarding changes in temperature, new text and a new table have been added to directly address this:

"Though phytoplankton productivity has declined, aspects of the changes in ocean physics should favour their growth. For instance, warmer temperatures permit higher maximum growth rates (Eppley, 1972), while shallower mixing confines phytoplankton cells within a thinner mixed layer and increases the average irradiance that they experience. Table X presents a summary of the changes in phytoplankton biomass, productivity and growth rates between the 1990s and 2090s. As already noted, both biomass and productivity decline across the 21st century, but this occurs against the backdrop of increasing potential growth rates, both for non-diatoms and diatoms. However, both modelled phytoplankton groups experience increasing nutrient limitation, and actual growth rates are markedly lower than potential ones. Nonetheless, in the case of non-diatoms, the increase in potential growth rates is sufficient to - on average - marginally offset increasing nutrient limitation, although this is not the case for model diatoms. Note that the average growth rates shown here are calculated using a weighting by biomass to avoid being skewed by those from warm, well-lit, but unproductive, regions such as ocean gyres."

Also, the authors state that the physical circulation field has some important deficiencies, such as a much too strong Antarctic Circumpolar Current, but just mention this as a caveat without further discussion on how this might affect model outcomes.

The deficiency in modelled ACC circulation was originally presented without any contextualising information. We have now added information from a CMIP5 analysis of model performance that places our NEMO simulation within a range of model behaviour. We have also added text to the section validating biogeochemical tracers that addresses the impacts on these. Finally, while it may be stating the obvious, we have added a sentence reminding readers that, because of such biases in models (not just this one), relative changes are more important than absolute changes:

"While the strength of the Antarctic Circumpolar Current (ACC) is estimated at round 130-140 Sv (Cunningham et al., 2003), in NEMO it is noticeably stronger at 220 Sv, and toward the high end of comparable models (CMIP5 range of 90-264 Sv; Meijers et al., 2012)."

"In terms of dissolved oxygen, the elevated ventilation means that the higher concentrations of this tracer that occur in surface waters are mixed down to abyssal depths in NEMO, with values around 50 mmol / m3 higher than those observed. Away from the Southern Ocean, discrepancies are less pronounced and Medusa-2.0's nutrient and oxygen distributions are more similar to those observed."

"In general, and in common with comparable models (cf. Steinacher et al., 2010; Meijers et al., 2012), NEMO and Medusa-2.0 present a number of discrepancies with observed quantities. As such, qualitative and relative changes are of greater significance than their absolute magnitudes."

I find it a pity that the effects of one of the really innovative characteristics of Medusa, namely the different C:N ratios in different trophic levels are not explored at all.

Though MEDUSA-2 has different C:N ratios in its trophic levels, these are held constant, unlike other models. We have, however, amended the manuscript to mention the change in C:N of total POC (i.e. P + Z + D) in the model with time and depth, and have noted MEDUSA-2's omission of variable C:N, which some studies would contend is a significant omission (e.g. Riebesell et al., 2007).

p. 3457, l. 28: That PP in the ocean is limited by nutrients is probably one of the important mechanisms, the other being top-down control by grazers; I would argue that it is probably the interplay between both. Also, on land, a large role is played by water, and the CO2-effect is related to better economies of water usage, if I am not mistaken.

We would agree that this statement oversimplifies matters and have amended it to the following:

"As dissolved inorganic carbon (DIC) is abundant in seawater, primary production in the ocean is not limited by CO2, but may instead be limited by the availability of nutrients such as nitrogen, silicon or iron (and by the activity of grazing zooplankton). As a result, the biological pump is not believed to have played any significant role in the ocean's response to anthropogenic CO2 to date"

p. 3458, l. 24ff: perhaps one could already mention the different isomorphs of CaCO3 here? I would hesitate to call CaCO3 a biomineral without mentioning its crystal form(s).

Again, we would agree with the referee's comment, and have amended the text to:

"This is widely-produced by marine organisms ranging across both the trophic- and size-spectra, from primary producers, such as coccolithophorids, through to heterotrophs, such as protistan foraminifera and metazoan pterpods. It is typically produced to form structures such as cell-coverings, shells or skeletons, and occurs in two major crystal forms, calcite and aragonite, that differ in their solubility."

p. 3460ff: It would be helpful to mention which processes in Medusa are directly temperature-dependent.

The following sentence has been inserted into the opening paragraph of MEDUSA-2's description:

"Within MEDUSA-2, phytoplankton growth and slow-sinking detritus remineralisation are temperature-dependent, with both exhibiting higher rates at warmer ambient temperatures (following Eppley, 1972)."

p. 3467, l. 25: Why compare against an average of the three PP algorithms?

This was originally done at the suggestion of a colleague as a short-hand way of comparing quite different estimates of primary production (all based on the same observational data) with model output in a single plot. To make this rationale clearer, we have added "For simplicity of presentation" to the sentence introducing the plots.

We have also prepared a figure to accompany this response (but which does not appear in the manuscript), that compares the range in observation-based estimates to the model discrepancy with the estimate mean. As this figure shows, model error is comparable with the range of estimated primary production, and there are areas where model error is noticeably smaller than the diversity in observational estimates.

p. 3468, l. 1: Is that an indication of deficits in the modeled iron distribution?

We are not quite sure to which part of line 1 the referee is referring. Regarding North Atlantic productivity, while iron plays a role here, nitrogen availability is noticeably lower than that observed. In the Southern Ocean, excessive ventilation of deep waters markedly increases all nutrient concentrations, including iron, which offsets its limited availability from dust deposition.

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p. 3468, l. 15: Does the model include riverine inflow of carbon? If I understood it correctly, the uptake of 1.35 Pg C (which btw. is the SI unit, not Gt) is the sum of 2 Pg C uptake of anthropogenic carbon and 0.6 PgC outgassing of river-derived carbon.

The model does not include riverine inputs (i.e. a no-flux boundary condition where river water implicitly has the same DIC concentration as the seawater it mixes into). Our net air-sea flux of 1.35 Pg C / y is entirely due to exchange between the modelled oceanic C pool and the unmodelled atmospheric C pool. Because of its low spatial resolution, as well as its open ocean maxima and minima, we have assumed that the Takahashi et al. (2009) net air-sea flux omits riverine inputs (i.e. outgassing of riverine DIC occurs nearshore). We will, however, amend the manuscript to use the correct units.

p. 3470, l. 15ff: If the changing iron distribution has a major effect on the changes in primary and export production then it would be helpful to have at least some information how iron is modeled (how is the benthic source parameterized, what is the assumed scavenging rate etc.).

As we would prefer to limit discussion of MEDUSA-2 in this manuscript, we have amended the text to remind the reader about our model description manuscript, and to give an overview of the sources of iron to the ocean:

“The situation is somewhat different for the micronutrient iron (Medusa-2.0 uses the submodel of Dutkiewicz et al., 2005; see Yool et al., submitted for more details). As well as being transported to the surface ocean by physical processes, this nutrient is directly supplied to the ocean in Medusa-2.0 by aeolian deposition (global; spatio-temporally variable) and by benthic supply (shallow water; spatio-temporally constant). As such, its patterns are distinct and different from those of macronutrients, and these differences continue into its future response to climate change (results not shown).”

p. 3471, l. 28-29: It is indeed likely that the decrease in PP is due to the nutrient changes shown. But what is the role of compensating effects, such as higher temper-

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ature or higher mixed layer irradiance in high latitudes?

Please see the earlier text on growth rates changes.

p. 3472, l. 20-24: How is the change in diatom Si:N ratio described in the model, as a pre-set function of iron limitation? Si:N racts also to nitrogen limitation, see e.g. Claquin et al. 2002.

The Si:N model used in MEDUSA-2 allows for impacts on both primary production and opal production depending upon the Si:N ratio. MEDUSA-2 does not directly include an iron impact on Si:N, but in regions where iron is limiting, N uptake is also limited, while Si uptake is not (within a certain range of Si:N). The text has been amended slightly to note this:

“This is driven by Medusa-2.0’s diatom silicon submodel (Mongin et al., 2006), which permits diatoms to continue to uptake silicic acid and synthesise opal even while biomass production is limited by nitrogen or iron availability.”

p. 3477, l. 4-6: If this is an important effect, then why is it not really shown/discussed before?

Essentially, because we judge some of our other findings as being of more scientific interest. However, the decline of North Atlantic production is still a striking feature of our simulation, so we have amended both the abstract and conclusions to reflect this.

p. 3477, l. 9: What do the percentages stand for?

This is the fraction of productivity that is driven by diatoms. The text has been amended to make this clearer.

Finally a brief note on one comment by reviewer 1: He/she asks (p. wC2080 line 1) why some equations use the "partial" symbol and others use the "d". This is actually correct, since the equations that use the "d" symbol are those for the benthic layer, where in the model no advection or diffusion takes place.

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We are grateful to the referee for clarifying this point.

p. 3466, l. 7: perhaps insert 'globally averaged' before SST?

Amended as directed.

page 3458, line 27: pterpods to pteropods

Amended as directed.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C3251/2013/bgd-10-C3251-2013-supplement.zip

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Interactive comment on Biogeosciences Discuss., 10, 3455, 2013.

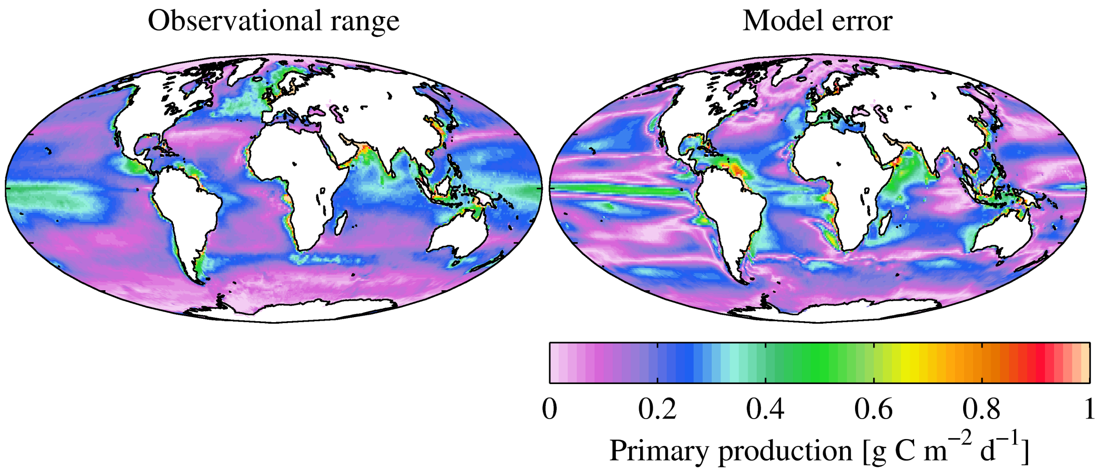
Observational range
Model error
0
0.2
0.4
0.6
0.8
1
Primary production [g C m^{-2} d^{-1}]

Fig. 1.